The polyps bear the gonads, and are apparently viviparous. Very interesting conclusions are arrived at by the authors by comparison of the various stages at their disposal as to the mode of growth and successive additions of fresh polyps to the colony around the terminal primary polyp, and these are at variance with those of Lindahl. A couple of lateral polyps appear on each side of the terminal polyp, then another pair of laterals are formed, and the rachis expands in breadth. The centro-dorsal polyp is formed, and then the dorso-lateral are developed, whilst the lateral polyps become more numerous.

H. N. MOSELEY

OUR BOOK SHELF

A Flora of the English Lake District. By J. G. Baker, F.R.S., F.L.S. (London: George Bell and Sons, York Street, Covent Garden, 1885.)

IT is perhaps surprising that a "Flora" of the Lake district has not before been issued, considering the large number of botanists who have yearly rambled over its fells and dales. It has been left to Mr. J. G. Baker to do so, and with modesty he says "it does not seem likely at present to stand in the way of anything more complete." The limits of the "Flora" embrace parts of Cumberland, Westmoreland, and the whole of what is botanically called Lake Lancashire; but excludes "the northern half of Cumberland and the western slope of the Pennine Chain, through Cumberland and Westmoreland;" the exact boundaries are, however, not very clearly defined

One cannot help feeling, directly the book is opened, that it is the work of one used to generalise and deal with facts in a broad way: in no part more so than in the first fourteen pages, where, accepting Mr. H. C. Watson's definitions, he describes the distributive types, zones of altitude, temperature, &c., with a clearness coming of long and practical acquaintance with the subject, giving comparative tables of the types, &c., with those of Northern Yorkshire, Northumberland and Durham, and Britain, and making the Lake Flora about 900 species. It should, however, be remembered that this number is based on Mr. H. C. Watson's estimate of 1425 species for Britain as a whole.

Had that estimate to be made *now* by Mr. Watson, the result would probably be the accepting of a larger number, not alone by the discovery of species 'since made, but by a decided feeling on his part "that there were some species that would eventually have to be divided." It may well be asked *why* is there this comparatively large amount of difference demanded among our native plants to constitute a "species," and the little often accepted among newly-discovered "species" from distant countries; doubtless knowledge is progressive in the latter case, but still theories and generalisations are built up on them with as much apparent certainty as on floras long known and studied. Mr. Baker then enumerates the species constituting the flora, running up to 234 pages, numbering them according to the sixth edition of the "London Catalogue," showing also (but not numbering) the large number of doubtful plants that have at various times been reported from the district.

Perhaps the most striking fact brought out by this "Flora" is the scarcity of aquatic species compared with the numerous lakes and tarns, of which there must be between sixty and seventy, large and small. Whether in this particular district this is from the want of investigation, or from a real paucity of species or specimens, is difficult to say; but certainly our lakes and waters have not been sufficiently systematically searched, whether from the botanical, zoological, or chemical point of view. In this we should do well to emulate the Swedish naturalists; but in *our* case it may well be asked, "Where are we to look for help?"

How little we know of the life-histories of our aquatic plants! and it may well be suggested as a study for those botanists, who, while not being able to take up botany in the way so ably advocated lately by Prof. Bower in NATURE, still have some leisure from other occupations and duties, and could really advance the knowledge of our flora beyond mere collecting. It is only necessary to turn over the plates of Dr. T. Irmisch's work on them to understand what is meant and required. AR. B.

The Fallacy of the Present Theory of Sound. By Henry A. Mott, jun., Ph.D., E.M., &c., Professor of Chemistry and Physics in the New York Medical College and Hospital for Women; Author of "The Chemist's Manual," "Was Man Created?" "Adulteration of Milk," "Testing the Value of Rifles by Firing under Water," "The Laws of Nature," "The Air We Breathe and Ventilations," &c. 12mo. (New York: Printed for the Author, 1885.)

THIS is a very curious book. Its author appends to his name recognised scientific titles, and seems to hold a responsible position as a teacher; but he has been led into a hopeless and inextricable muddle about wave-motion; and, starting with a misconception, he naturally obtains results so utterly at variance with common sense and experience, that it is remarkable he cannot see his error.

He begins by admitting that "to attack a theory which has been upheld for 2500 years, and which has been and is sustained by the greatest living scientists, is certainly a very bold undertaking." But he feels bound, nevertheless, "to come to the front and join Dr. A. Wilford Hall in exposing the fallacy." He fulminates, moreover, the following withering defiance at false prophets: "If Profs. Helmholtz, Tyndall, Lord Rayleigh, Sir William Thomson in Europe, and Profs. Rood and Mayer in this country, wish to retain the respect and confidence of thinking people, they will at once endeavour either to defend the theory of sound, or, like men, come boldly to the front and acknowledge that it is fallacious."

There can be no doubt that these various noblemen and gentlemen will at once proceed to adopt humbly the latter and safer alternative; because it is obvious that if they do not do so speedily, creation and nature will come to a premature end. This rather serious occurrence is thus predicted: "The lowest tone of an organ is stated by Prof. Blaserna to have sixteen vibrations to the second, and a consequent wave-length of 70 feet. It thus follows, says Dr. Hall, that in the sound of such an organ-pipe the air-particles (as a whole) are obliged to travel 35 feet and back sixteen times each second in order to pass from the space occupied by the centre of rarefaction to the centre of condensation and back. They would thus move with a velocity in one direction of 560 feet a second, or at the rate of 381 miles an hour, which would produce a tornado of more than double the velocity necessary to sweep a village into ruins. If there was the least truth in the wave-theory, the sound of a church-organ should get up a cyclone which would blow a cathedral into atoms."

This is truly very horrible! far worse than dynamite. Saddened by these reflections, we can bear with comparative equanimity the revelation that "the prong of a tuning-fork moves at the rate of only about one inch in four years," and "instead of swiftly advancing, as Tyndall says, sounds audibly when moving more than 25,000 times slower than the hour hand of a family clock, and more than 300,000,000 times slower than any clock-pendulum ever constructed, instead of very much faster, as Helmholtz teaches."

One more quotation is irresistible: "Imagine," says our author, who seems to have recovered wonderfully from the terrestrial cataclysm which he and the evil-doers

above named have all but provoked, "imagine a locust stridulating in the centre of a mass of iron one mile in all directions" (*sic*). The idea is charming, countrified, bucolic, but perhaps rather cold for the poor insect! "It is admitted he could be heard, and about sixteen times quicker than in the air. . . ." (the steps of this grand calculation must perforce be omitted). "The mass of iron thus displaced" (*i.e.* by said locust) "would weigh not less than 729,749,050,612 tons, and would be so moved by the strength of the locust."

The thought is too tremendous! so, locust-like, I must cease to "stridulate," lest I bring down the solar system in ruins on the heads of innocent humanity.

W. H. STONE

Kryptogamen-Flora von Schlesien. Vol. III.: *Pilze.* Bearbeitet von Dr. J. Schröter. (Breslau: J. U. Kern.)

DR. COHN'S "Cryptogamic Flora" is already so favourably known by the portions which have appeared, that the announcement of any subsequent part is sure to be received with satisfaction. The first part of the Fungi, by Dr. J. Schröter, is just issued, and consists almost entirely of an introduction of nearly 100 pages, carefully digested and summarised, concluding with the order of classification adopted. The three groups or primary divisions are:—(1) Myxomycetes; (2) Schizomycetes; and (3) Eumycetes. The latter embraces the Chytridiei, Zygomycetes, Oomycetes, Protomycetes, Ustilaginei, Uredinei, Auriculariei, Basidiomycetes, and Ascomycetes, with an appendix for the incomplete Hyphomycetes, Tuberculariei, and Sphæropsidei. As the present part contains only a portion of the Myxomycetes, no opinion can be formed of the manner in which the foregoing skeleton will be filled up; but, as this portion is based upon Rostafinski's monograph, no exception can be taken thereto. The real difficulty lies further ahead, and whether the knot is to be untied or cut cannot be predicted.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Terminology of the Mathematical Theory of Elasticity

I HAVE been greatly interested by the letters on this subject from Prof. K. Pearson (NATURE, vol. xxxi. p. 456) and Prof. A. B. W. Kennedy (vol. xxxi. p. 504), and I have looked forward with pleasure to further communications from other eminent "elasticians." As, however, no better qualified person seems disposed to continue the correspondence, and as I am practically interested in a definite settlement of elastic terminology, I venture to offer a few remarks on the subject.

(1) Nothing could be better than Prof. Pearson's term *state of ease* for the condition of an elastic body when capable of enduring a certain amount of stress, without showing permanent set on its removal. This is worthy of Clifford, and is sure to make its way.

Prof. Kennedy has extended this term by applying "maximum state of ease" to the condition in which the body may be strained to its elastic limit without set. Perhaps *ultimate state of ease* would be more significant, and *limited state of ease* might be employed to denote the intermediate stages. The ultimate state of ease of course corresponds to the "natural state" of the ideal perfectly elastic solid.

At the point B in Prof. Kennedy's figure we reach the *limit of perfect elasticity*, and enter the stage b of *elastic instability*. Prof. Kennedy's suggestion of "limit of stability" for the point C is inconsistent with the last. I would suggest *elastic crisis* as an alternative for "breaking-down point." We evidently here pass the critical point in the static equilibrium of the molecules.

Perhaps c' might be called the stage of *thermal inversion*.

At C_1 the bar enters the *plastic state*—divided by Prof. Kennedy into the *stage of uniform flow* from C_1 to the point D of *maximum load* and the *stage of local flow* from D to the point E of *terminal load* or (apparently) of *maximum stress*.

(2) I observe that Prof. Kennedy uses "load" and "external stress," apparently as alternative terms, and that Prof. Pearson speaks of "stress per unit area." Would it not be advisable to settle, once for all, that *stress* shall always, when it stands alone, mean a force per unit area? "Stress" and "intensity of stress" would then be identical terms, while the *force* across a given area due to stress would be known as the "total" or "resultant stress" across the area. This is all that is required to bring the terminology of *perfect* elasticity into exact correspondence with that of hydromechanics, in which pressure and total or resultant pressure have always stood in this relation to one another.

(3) Next as to "tension." The word was originally adopted from the theory of strings, and of bars used like strings to support weights, and was, I believe, invariably used (as it still is in the case of strings) to denote the load, or *total* longitudinal stress endured. Nowadays, however, it seems to be employed indifferently in this sense and in that of intensity of tensile stress. I would suggest that the term *traction*, which the modern French writers have freely adopted, should be invariably used to denote intensity of tensile stress, and that *tension* should be restored to its original signification of total or resultant traction.

"Traction" and "pressure" would then (according to the ordinary convention as to sign) be synonymous with "positive" and "negative" stress. Perhaps some elastician would suggest a convenient abbreviation for "total pressure" or "negative tension."

(4) Is it too late to protest on behalf of that much-abused term *viscosity* as applied to solids? The thoroughly-established sense of the word, as applied to fluids, implies—*not* the property in virtue of which they undergo permanent or continued change of shape under continued distorting stress (*i.e.* their *fluidity*); but that other property in virtue of which they are able to offer more or less resistance, by means of molecular friction, to instantaneous changes of shape under stress which is not continued. In this case, therefore, viscosity is a property distinctly opposed to fluidity, and, indeed, described in terms as a falling short of "perfect fluidity."

It is thus obviously false analogy to describe a metal in the state of plastic flow as *viscous*, or to "appropriate this name to that permanent set which may be produced by the application for a long period of a stress well within the limits of elasticity." The latter sense—at least as applied to ice (NATURE, vol. xxxii. p. 16)—has, no doubt, a classical authority in the great memoir of Forbes; but Sir W. Thomson has pointed out ("Enc. Brit.," Art. "Elasticity," § 31; and Thomson and Tait's "Natural Philosophy," § 741) that the properties of ice so described are included under the perfectly definite and convenient term *plasticity*, which is really analogous to fluidity.

On the other hand, analogy demands that the term *viscosity*, as applied to solids, shall be strictly confined to that frictional dissipation of energy which is always at work during rapid changes of strain, and which was first discovered during small vibrations within the elastic limit by Sir W. Thomson (*Proc. Roy. Soc.*, May 18, 1865, or the passages above cited).

That the viscosity of a ductile material is very greatly increased in the plastic stage is of course beyond a doubt, the amount of energy absorbed by it on sudden increase of the stress being so much in excess of that required to provide for the increased potential energy of the accompanying strain that the temperature rises to a sensible extent. But what I wish to make clear is that the true viscosity is not essential to or characteristic of the truly plastic state, but that, on the contrary, the viscosity of a ductile solid renders it *imperfectly plastic* in just the same sense as a viscous fluid is *imperfectly fluid*.

(5) Finally, I may perhaps be permitted to add that, next to the importance to all concerned of a definite and universal terminology, comes the importance to mathematicians at least of a uniform *notation*.

The effect of reading through, for purposes of comparison or historical record, the 100 odd *really important* treatises on this subject—in each of which a perfectly independent and generally